cism misses the mark. It need only be stated that Castle was wrong in supposing that the cross-overs between yellow and white should be subtracted from the total before the white bifid cross-over value is calculated.

If, as Castle states, he cannot conceive of a mechanism "which would tie two genes together in such a way that they will subsequently separate from each other oftener than they will remain together, yet this is what the idea of cross-overs in excess of 50 per cent amounts to," it would seem to follow that he has not really understood the mechanism that we have described, and which he has attacked; for, whether such a mechanism really exists or not, it is nevertheless a conceivable mechanical device that could do just this thing.

We have left to Dr. Metz the opportunity to answer Castle's criticism relating to D. virilis.

To sum up: we believe that we have met all the pertinent criticisms that Castle has brought forward of our methods and conclusions, and that he has failed to meet our criticism of his three dimensional model.

THE ARRANGEMENT OF GENES IN DROSOPHILA VIRILIS

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Communicated by T. H. Morgan, March 6, 1920

In connection with his general attack on the theory of the linear arrangement of genes Castle[1] has questioned my use of this theory in explaining the genetical results obtained in Drosophila virilis. I did not consider it necessary to reply to this criticism because Castle's general position was shown by Sturtevant, Bridges and Morgan[2] to be untenable as regards Drosophila melanogaster, with which D. virilis agrees in its mode of inheritance. Subsequently, however, Castle has reaffirmed his belief in the superiority of his hypothesis over that of linear arrangement[3] and has apparently misconstrued the silence on my part with respect to Drosophila virilis. This would seem to call for a brief reply.

From my paper on eight sex-linked characters in D. virilis[4] Castle concluded that the genes dealt with could not be arranged in a linear series, and by applying his three dimensional hypothesis to the case he made certain predictions (*b*, p. 36) which he now wishes used as a test of his hypothesis. His general arguments in this case are the same as those he used previously in reference to Drosophila melanogaster, and since they are being treated by those toward whom they were first directed, I will avoid repetition by confining myself to his predictions. These apply to four undetermined cross-over values in D. virilis, namely, magenta-hairy, glazed-rugose, frayed-forked and frayed-glazed.

It should be pointed out first that in making these predictions Castle apparently overlooked the statement in my paper (pp. 113 and 125) that the frayed stock had been lost shortly after it was obtained. The absence

of this stock makes it impossible to test the latter two of his predictions. The other two predictions are also such that they cannot be tested at present; the first because of the loss of hairy stock through sterility, and the second because of the sterility of rugose-glazed hybrids.[5] Had it not been for these practical obstacles the values would not have been left undetermined in my original paper.

The matter need not be dropped here, however. It should be noted that, in any event, tests of the predictions would have been futile, for the two hypotheses may be compared just as well by means of known values as by testing unknown ones. In addition, the very nature of the predictions themselves prevents their being used for critical tests, because they are so constructed that, on either view, they must be fulfilled if my data give sufficiently accurate ratios.[6] There is no alternative. This being the case it becomes a question of ascertaining the accuracy of my data[7] or the validity of the assumptions underlying Castle's predictions.

An illustration may be used to amplify this point. Castle's last two predictions apply to the gene for frayed. My data on frayed (pp. 112 and 126) as tabulated by Castle would locate the gene 1.3 units from yellow and 18.6 units from vesiculated. Castle's own calculation of the value yellow-vesiculated is 17.4. This would put the three genes in almost exactly a straight line (1.3 + 17.4 = 18.7, as compared with 18.6). These are the only data available for the direct determination of the location of frayed, but so far as they go they conform to the linear hypothesis. Castle's predictions relate to the cross-over values that should be given by frayed and forked, and by frayed and glazed. The former should be "between 39 and 41" and the latter "between 43 and 46." The predicted values are, of course, to be calculated entirely on the basis of single cross-overs, as all values are determined on his system.

These predictions are evidently based on the cross-over values given by yellow and forked, and yellow and glazed, respectively. By leaving out of account the double cross-overs, Castle calculated the latter values from my data as 40 and 44.5. Since frayed is approximately one unit from yellow it should give within approximately one unit of the same cross-over values as yellow. This principle would apply on either hypothesis. It is evident, then, that by calculating the value frayed-forked in the same manner that yellow-forked was calculated—namely, by using only single cross-overs—the result would have to be within approximately one unit of 40, if crossing over is a consistent process. The same principle applies to the frayed-glazed relation. Both predictions would have to be fulfilled (within the limits of experimental error), under these conditions. But this does not in any way substantiate the three dimensional hypothesis. Predictions in such cases as these can be made and fulfilled on either hypothesis, providing the calculations are made in accordance with the hypothesis (the one including, the other excluding, double cross-overs).

It is not necessary to carry the analysis farther, since neither theory would be benefitted by the fulfillment or non-fulfillment of these predictions. The question at issue is not met in this manner. It is involved in features lying outside the province of the present paper and is being treated by other authors, as mentioned above.

[1] Castle, W. E., (*a*) "Is the arrangement of the genes in the chromosome linear?" these PROCEEDINGS, **5** 1919 (25–32); (*b*) "The linkage system of eight sex-linked characters in Drosophila virilis (data of Metz)," *Ibid.*, **5** (32–36).

[2] Sturtevant, Bridges and Morgan, "The spatial relations of genes," *Ibid.*, **5**, 1919 (168–173).

[3] Castle, W. E., "Are genes linear or non-linear in arrangement?" *Ibid.*, **5**, 1919 (500–506).

[4] Metz, C. W., "The linkage of eight sex-linked characters in Drosophila virilis," *Genetics*, **3**, 1918 (107–134).

[5] See Metz and Bridges, these PROCEEDINGS, **3**, 1917 (673–678). Before the hairy stock was lost some data on the value hairy-magenta were obtained by Weinstein, as noted by Muller (*Amer. Nat.*, **54**, p. 118), but the numbers were hardly large enough to be conclusive.

[6] Assuming, of course, that crossing-over is a consistent process, and that the experiments are conducted under similar conditions.

[7] My data, in the cases under consideration, include only relatively small numbers and give ratios of only approximate accuracy, as indicated in my paper. This fact alone would vitiate predictions of short distances (such as the first two) even if they were otherwise valid.

THE FUNCTIONS OF INTENSITY AND PHASE IN THE BINAURAL LOCATION OF PURE TONES

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The permanence of the value of knowledge concerning the physical aspects of audition gives to contributions in this field a greater importance that is at first apparent. There has been much confusion as to the factors involved in the binaural location of sounds. An analysis of the problem of ascertaining the important factors and their relative values show that the solution will be secured most speedily by the separate control of these factors and by using first pure and then complex tones. This report includes experiments covering the frequency range of 100 to 1200 d.v., the results giving the effects of intensity differences and phase differences at the ears, each factor treated first alone and then in combination.

Intensity.—Rayleigh (*Phil. Mag.*, **13**, 1907 (214)) was the first to point out that for frequencies of 128 and 256 d.v., the difference in intensity at the ears could not account for the ability to locate the source of sound. Although there have been many experimental contributions in the localization of sound, the only observers to make quantitative measurements of the effect of intensity were Hovda and the writer (*Psych. Rev.*, **25**, 1918,